PHILOSOPHICAL

TRANSACTIONS.

I. *Of the Methods of manifesting the Presence, and ascertaining the Quality, of small Quantities of Natural or Artificial Electricity. By Mr.* Tiberius Cavallo, *F. R. S.*

The Lecture founded by the late Henry Baker, Esq. F. R. S.

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THE persevering industry of many ingenious persons in the philosophical world, since the beginning of the present century, has gradually, and with rapid success, examined the effects, ascertained the properties, and investigated

the laws of a ſcience almoſt entirely unknown before. Various wonderful phænomena of nature have been explained, means have been diſcovered of avoiding ſome of their dangerous effects, and a greater inſight into the general operations of nature has been derived, from the knowledge of ſo great and ſo extenſive a power as is commonly underſtood under the name of *electricity*; yet, if inſtead of contemplating its more ſtriking phænomena, *viz.* thoſe which are apt to ſurpriſe more by the magnitude of their effects, than by the inſtruction which they may afford, we endeavour to examine the ſubject more cloſely, we ſhall find that our knowledge of electricity goes very little, if at all, beyond the ſuperficial part of it. We know, for inſtance, that a piece of glaſs, or other electric, when rubbed, will produce that power which we call electricity, that the glaſs will communicate the acquired electricity to a piece of metal, that the piece of metal will retain that power in certain circumſtances, and ſo on; but no perſon has ſhewn how that power is generated by the friction, or what prevents its paſſage through the ſubſtance of ſome particular bodies. It has been aſcertained, that the air of moſt countries, and probably of the whole world, as well as the clouds, fogs, rains, &c. are almoſt always electrified; but we are ignorant of the office which this electricity can have in the great laboratory of nature; for ſurely ſo general and ſo active a power can hardly be intended by nature, merely to intimidate mankind now and then with the thunder and the lightning.

It appears, therefore, that thoſe perſons, who are now willing to diſtinguiſh themſelves in this intereſting branch of natural philoſophy, ought to examine the electrical power not ſo much in its accumulated, as in its incipient ſtate. Its firſt

origin, or very beginning, ought to be inveſtigated, it being afterwards very eaſy to underſtand its increaſe; for we may eaſily comprehend, how a great quantity of it may be accumulated from the often repeated additions of its ſmalleſt portions.

This truth having been underſtood by ſeveral philoſophical perſons, has induced them to contrive inſtruments neceſſary for the purpoſe, and to make numerous obſervations, which have undoubtedly promoted the knowledge of the ſubject; but a great deal ſtill remains to be done, before we can attain to the knowledge of the object in view, *viz.* of the real nature, of the firſt origin, and of the general uſe of electricity. The inſtruments hitherto invented are ſtill inadequate to the purpoſe, and the known methods of operating are not free from conſiderable objections. To examine the peculiar conſtructions, intended uſes, properties, and defects, of thoſe inſtruments, as well as methods of performing the experiments, is the principal object of the preſent Lecture, which I have the honour of delivering to this learned Society, and which may, perhaps, be of uſe to other experimenters in this branch of philoſophy; ſince what is mentioned in the following pages is the reſult of long experience, and of conſideration. Of experience, which has often contradicted the ſanguine expectations of a pre-conceived theory; and of conſideration, which has frequently demonſtrated the abſurdity or inſufficiency of ſchemes which, at firſt ſight, ſeemed very eaſy and plauſible.

The late Mr. JOHN CANTON was, as far as I know, the firſt perſon who conſtructed an electrometer, or inſtrument capable of ſhewing the preſence of what was then conſidered as a ſmall quantity of electricity. This inſtrument conſiſted of two ſmall balls of pith of elder or of cork, faſtened to the

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two extremities of a linen thread, the middle of which was faſtened to an oblong wooden box, in which the thread and balls were kept, when not actually in uſe. It was with ſuch an inſtrument that Mr. CANTON himſelf, Father BECCARIA, and others, aſcertained the electricity generally exiſting in the air; and that Mr. RONAYNE diſcovered the conſtant electricity of fogs. But in the courſe of my experiments, having made frequent uſe of ſuch electrometers, it naturally occurred to me, that in ſeveral caſes, when the electrometer gave no ſigns of electricity, or at leaſt not ſufficient to aſcertain its quality, the cauſe of it was the reſpectively large ſize of the inſtrument; for a ſmall quantity of electricity being diffuſed through the box, thread, and balls of the electrometer, had not power ſufficient to ſeparate the balls, and of courſe to ſhew its preſence. In conſequence of this, I contracted the ſize of the electrometers to ſuch a degree as could be affected by leſs than the tenth part of that quantity of electricity which was neceſſary to affect Mr. CANTON's electrometer. But in making the electrometers very ſhort, the ſtiffneſs of the threads, which had been inſignificant in a great extent, became now very conſiderable; hence, inſtead of faſtening the balls to the two extremities of one piece of thread, I found it neceſſary to ſuſpend each ball by a ſeparate piece of thread, the upper part of which was formed into a loop, which moved in a ring of braſs wire.

The electrometers, thus improved, were ſtill ſubject to a very great imperfection, which was the twiſting of the threads; to avoid which I ſubſtituted fine ſilver wire, inſtead of linen threads, which anſwered very well. However, in obſerving the electricity of the atmoſphere, thoſe electrometers appeared to labour under a conſiderable inconvenience, which was their

being diſturbed by the wind; for it frequently happened, that neither the exiſtence, nor the quality, of the electricity could be aſcertained on account of the wind agitating the inſtrument, and becauſe the electricity being in ſmall quantity would not remain on the electrometer a time ſufficient to bring the inſtrument within doors, where it might be examined without any obſtruction. In order to remove this imperfection, I incloſed the electrometer in a bottle, as it is deſcribed in the Philoſophical Tranſactions, Vol. LXX. which conſtruction has been found to anſwer remarkably well.

This bottle electrometer has been ſince altered by various perſons; though, in my opinion, thoſe alterations do not tend to improve it altogether. M. DE SAUSSURE, by altering the ſhape of the bottle, and depriving it of a neck, has rendered it capable of retaining the communicated electricity only for a very ſhort time*; whereas, ſome of thoſe electrometers, conſtructed on my original plan, have retained the communicated electricity for more than four hours.

Beſides this, M. DE SAUSSURE, as well as ſome who make theſe electrometers for ſale, have ſubſtituted pith balls, inſtead of the conical corks of the original plan, the latter of which are preferable for two reaſons; *viz.* firſt, becauſe the balls of pith are apt to adhere to each other, ſo that on communicating the electricity they often do not ſhew any repulſion at all; or if the communicated electricity be conſiderably great, the pith balls, after adhering to each other for ſome time, are at laſt ſeparated with violence, ſo as to fly to the ſides of the bottle, which fruſtrates the experiment; whereas the corks are not near ſo ſubject to this inconvenience; and, ſecondly, becauſe the conical ſhape renders thoſe corks capable of preſenting a much

* SAUSSURE's Voyage dans les Alpes, Tom. II.

greater ſurface to each other, than if they were globular, their weights being the ſame in both caſes; for it is very well known, that, of all the ſolid figures, the ſphere contains the greateſt capacity under the leaſt ſurface; and as the effects of electricity, in thoſe caſes, is proportionate to the ſurface, and not to the quantity of matter, it follows, that the conical ſhape is preferable to the globular.

Another alteration of the bottle electrometer was lately made by the Rev. Mr. BENNET, and is deſcribed in the Philoſophical Tranſactions, Vol. LXXVII. Part I. It conſiſts principally in ſubſtituting two ſlips of gold-leaf to the corks ſuſpended by wires. This alteration has ſome peculiar advantages and diſadvantages. Its advantages are in general a greater degree of ſenſibility, and a more eaſy conſtruction. Its diſadvantages are, firſt, that the inſtrument is not portable; and, ſecondly, that even when not carried about, it is apt to be ſpoiled very eaſily. However, in ſome caſes it is very uſeful, ſo that, upon the whole, it may be conſidered as a very good improvement.

As the faſtening of the ſlips of gold-leaf to this electrometer, and to let them hang parallel, is rather difficult, it will not be improper to deſcribe a method which I have contrived, in order to accompliſh it more eaſily. When the ſlips are cut, and are lying upon paper or leather, I make them equal in length, by meaſuring them with a pair of compaſſes, and cutting off a portion from the longeſt: then cut two bits of very fine paper, each about half an inch long, and a quarter of an inch broad, and ſtick one of them, by means of a little wax, which is rubbed upon them, to one extremity of each ſlip of gold-leaf, ſo as to form a kind of letter T. This done, I hold up, in the fingers of one hand, one piece of paper, with the gold leaf ſuſpended to it, and hold the other with the other hand; then

then bring them near to each other, and after having adjuſted them properly, ſo that the ſlips may hang parallel and ſmooth, I force the pieces of paper, which now touch each other, between the two ſides of a ſort of pincers made of braſs wire, which are faſtened to the under part of that piece which forms the top of the electrometer: and, in order to change thoſe ſlips when ſpoiled, I keep in a book ſeveral ſlips of gold-leaf ready cut, and furniſhed each with a piece of paper; ſo that by this means this electrometer is rendered, in a certain manner, portable.

Beſides the way of aſcertaining ſmall quantities of electricity, by means of very delicate electrometers, two methods have been communicated to the philoſophical world, by which ſuch quantities of electricity may be rendered manifeſt, as could not be perceived by other means. The firſt of thoſe methods is an invention of M. VOLTA, the apparatus for it being called the *Condenſer of Electricity*, and is deſcribed in the Philoſophical Tranſactions, Vol. LXXII. The ſecond is a contrivance of the above-mentioned Mr. BENNET, who calls the apparatus *The Doubler of Electricity*. A deſcription of it is inſerted in the Philoſophical Tranſactions, Vol. LXXVII.

M. VOLTA's condenſer conſiſts of a flat and ſmooth metal plate, furniſhed with an inſulating handle, and a ſemi-conducting, or imperfectly inſulating, plane. When one wiſhes to examine a weak electricity with this apparatus, as that of the air in calm and hot weather, which is not generally ſenſible to an electrometer, he muſt place the above-mentioned plate upon the ſemi-conducting plane, and a wire, or ſome other conducting ſubſtance, muſt be connected with the metal plate, and muſt be extended in the open air, ſo as to abſorb its electricity; then, after a certain time, the metal plate muſt be

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ſeparated from the ſemi-conducting plane, and being preſented to an electrometer will electrify it much more than if it had not been placed upon the above-mentioned plane.

The principle on which the action of this apparatus depends is, that the metal plate, whilſt ſtanding contiguous to the ſemi-conducting plane, will both abſorb and retain a much greater quantity of electricity than it can either abſorb or retain when ſeparate, its capacity being increaſed in the former, and diminiſhed in the latter caſe.

Whoever conſiders this apparatus, will eaſily find, that its office is not to manifeſt a ſmall quantity of electricity, but to condenſe an expanded quantity of electricity into a ſmall ſpace; hence, if by means of this apparatus one expected to render more manifeſt than it generally is, when communicated immediately to an electrometer, the electricity of a ſmall tourmalin, or of a hair when rubbed, he would find himſelf miſtaken.

It is Mr. BENNET's doubler that was intended to anſwer that end; *viz.* to multiply, by repeated doubling, a ſmall, and otherwiſe unperceivable, quantity of electricity, till it became ſufficient to affect an electrometer, to give ſparks, &c. The merit of this invention is certainly conſiderable; but the uſe of it is far from preciſe and certain.

This apparatus conſiſts of three braſs plates, which we ſhall call A, B, and C; each of which is about three or four inches in diameter. The firſt plate A is placed upon the gold-leaf electrometer, or it may be ſupported horizontally by any other inſulating ſtand, and its upper part only is thinly varniſhed. The ſecond plate B is varniſhed on both ſides, and is furniſhed with an inſulating handle, which is faſtened laterally to the edge of it. The third plate C is varniſhed on the under ſide

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only, and is furnished with an insulating handle, which is perpendicular to its upper surface.

This apparatus is used in the following manner. The plate B being laid upon the plate A, the small quantity of electricity, which is required to be multiplied, is communicated to the under part of the plate A, and at the same time the upper part of B is touched with a finger; then the finger is first removed; the plate B is afterwards removed from over the plate A. The plate C is now laid upon B, and its upper surface is touched, for a short time, with a finger. By this operation it is clear, that if the electricity communicated to the plate A is positive, the plate B must have acquired a negative electricity, and the plate C must have acquired the positive, *viz.* the same of the plate A. Now the plate B, being separated from C, is laid as before upon A; the edge of C is brought into contact with the under part of the plate A, and at the same time the upper part of B is touched with a finger, by which means the plate B, being acted upon by the atmospheres of both the plates A and C, will acquire nearly twice as much electricity as it did the first time, and of course will render the plate C, when that is laid upon it, proportionably more electrified than before: thus, by repeating this operation, the electricity may be increased to any required degree.

The varnish on those surfaces of the plates which are to lie contiguous to each other serves to prevent the metal of one touching the metal of the other; for in that case, instead of one plate causing a contrary electricity in the other, the electricity of the first would be gradually communicated to the others, and would be dissipated.

As soon as I understood the principle of this contrivance, I hastened to construct such an apparatus, in order to try several

experiments of a very delicate nature, eſpecially on animal bodies and vegetables, which could not have been attempted before, for want of a method of aſcertaining exceedingly ſmall quantities of electricity; but, after a great deal of trouble, and many experiments, I was at laſt forced to conclude, that the doubler of electricity is not an inſtrument to be depended upon, for this principal reaſon; *viz.* becauſe it multiplies not only the electricity which is willingly communicated to it from the ſubſtance in queſtion; but it multiplies alſo that electricity which in the courſe of the operation is almoſt unavoidably produced by accidental friction; or that quantity of electricity, however ſmall it may be, which adheres to the plates in ſpite of every care and precaution.

Having found, that with a doubler conſtructed in the above deſcribed manner, after doubling or multiplying twenty or thirty times, it always became ſtrongly electrified, though no electricity had been communicated to it before the operation, and though every endeavour of depriving it of any adhering electricity had been practiſed; I naturally attributed that electricity which appeared after repeatedly doubling, to ſome friction given to the varniſh of the plates in the courſe of the operation. In order to avoid entirely this ſource of miſtake, or at leaſt of ſuſpicion, I conſtructed three plates without the leaſt varniſh, and which, of courſe, could not touch each other, but were to ſtand only within about one-eighth of an inch of each other. To effect this, each plate ſtood vertical, and was ſupported by two glaſs ſticks, which were covered with ſealing-wax: ſee Tab. I. fig. 1. and 2., where AB is a wooden pedeſtal, $7\frac{1}{2}$ inches long, $2\frac{1}{2}$ broad, and $1\frac{1}{4}$ inch thick; C and D are the two glaſs ſticks cemented into the ſtand or pedeſtal AB, and likewiſe into the piece of wood E, which is faſtened to the

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back of the plate. The plate itself is of strong tin, and measures about eight inches in diameter. The stand AB projects very little before the plate, by which means, when two of those plates are placed upon a table facing each other, the wooden stands will prevent their coming into actual contact, as may be clearly perceived in fig. 3.

I need not describe the manner of doubling or of multiplying with those plates; the operation being essentially the same as when the plates are constructed according to Mr. BENNET's original plan, excepting that, instead of placing them one upon the other, mine are placed facing each other; and in performing the operation they are laid hold of by the wooden stand AB; so that no friction can take place either upon the glass legs, or upon any varnish; for these plates have no need of being varnished. Sometimes, instead of touching the plates themselves with the finger, I have fixed a piece of thin wire to the back of the plate, and have then applied the finger to the extremity of the wire, suspecting that some friction and some electricity might possibly be produced when the finger was applied in full contact to the plate itself.

It is evident, that as the plates do not come so near to each other in this, as they do in the other construction, the electricity of one of them cannot produce so great a quantity of the contrary electricity in the opposite plate; hence, in this construction, it will be necessary to continue the operation of doubling somewhat longer; but this disadvantage is more than repaid by the certainty of avoiding any friction.

Having constructed those plates, I thought that I might proceed to perform the intended experiments without any further obstruction; but in this I found myself quite mistaken: for, on trying to multiply with those new plates, and when no

electricity had been previouſly communicated to any of them, I found, that after doubling ten, fifteen, or at moſt twenty times, they became ſo full of electricity as to afford even ſparks. All my endeavours to deprive them of electricity proved ineffectual. Neither expoſing them, and eſpecially the glaſs ſticks, to the flame of burning paper, nor breathing upon them repeatedly, nor leaving them untouched for ſeveral days, and even for a whole month, during which time the plates remained connected with the ground by means of good conductors, nor any other precaution I could think of, was found capable of depriving them of every veſtige of electricity; ſo that they might ſhew none after doubling ten, fifteen, or at moſt twenty times.

The electricity produced by them was not always of the ſame ſort; for ſometimes it was negative for two or three days together; at other times it was poſitive for two or three days more; and often it changed in every operation. This made me ſuſpect, that poſſibly the beginning of that electricity was derived from my body, and being communicated by the finger to the plate that was firſt touched, was afterwards multiplied. In order to clear this ſuſpicion, I actually tried thoſe plates at different times, *viz.* before and after having walked a great deal, before and after dinner, &c. noting very accurately the quality of the electricity produced each time; but the effects ſeemed to be quite unconnected with the above-mentioned concomitant circumſtances, which independence was further confirmed by obſerving that the electricity produced by the plates was of a fluctuating nature, even when, inſtead of touching the plates with the finger, they had been touched with a wire, which was connected with the ground, and which I managed by means of an inſulating handle.

At laſt, after a great variety of experiments, which it is unneceſſary to deſcribe, I became fully convinced, that thoſe plates did always retain a ſmall quantity of electricity, perhaps of that ſort with which they had been laſt electrified, and of which it was almoſt impoſſible to deprive them. The various quality of the electricity produced was owing to this, *viz.* that as one of thoſe plates was poſſeſſed of a ſmall quantity of poſitive electricity, and another was poſſeſſed of the negative electricity, that plate which happened to be the moſt powerful, occaſioned a contrary electricity in the other plate, and finally produced an accumulation of that particular ſort of electricity.

Thoſe obſervations evidently ſhew, that no preciſe reſult can be obtained from the uſe of thoſe plates, and of courſe that, when conſtructed according to the original plan, they are ſtill more equivocal, becauſe they admit of more ſources of miſtake.

As thoſe plates, after doubling or multiplying only four or five times, ſhew no ſigns of electricity, none having been communicated to them before, I imagined that they might be uſeful ſo far only, *viz.* that when a ſmall quantity of electricity is communicated to any of them in the courſe of ſome experiment, one might multiply it with ſafety four or five times, which would even be of advantage in various caſes; but in this alſo my expectations were diſappointed, as will appear from the following pages.

Having obſerved, after many experiments, that, *ceteris paribus*, when I began to multiply from a certain plate, for inſtance A, the electricity which reſulted was generally poſitive; and when I began with another plate B, *viz.* conſidered this plate B as the firſt plate, the reſulting electricity was generally negative;

negative; I communicated ſome negative electricity to the plate A, with a view of deſtroying its inherent poſitive electricity. This plate A being now electrified negatively, but ſo weakly as juſt to affect an electrometer, I began doubling; but after having doubled three or four times, I found, by the help of an electrometer, that the communicated negative electricity in the plate was diminiſhed inſtead of being increaſed; ſo that ſometimes it vaniſhed entirely, though by continuing the operation it often began to increaſe again, after a certain period. This ſhews, that the quantity of electricity, which however ſmall it may be, remains in a manner faſtened to the plates, will help either to increaſe or to diminiſh the accumulation or multiplication of the communicated electricity, according as it happens to be of the ſame, or of a different nature.

After all the above-mentioned experiments made with thoſe doubling or multiplying plates, we may come to the following concluſion, *viz.* that the invention is very ingenious, but their uſe is by no means to be depended upon. It is to be wiſhed, that they may be improved, ſo as to obviate the weighty objections that have been mentioned in the preceding pages, the firſt deſideratum being to conſtruct a ſet of ſuch plates as, when no electricity is communicated, they will produce none after having performed the operation of doubling for a certain number of times.

Upon the whole, the methods by which ſmall quantities of electricity may be aſcertained with preciſion are, as far as I know, only three. If the abſolute quantity of electricity be ſmall and pretty well condenſed, as that produced by a ſmall tourmalin when heated, or by a hair when rubbed, the only effectual method of manifeſting its preſence, and aſcertaining its quality, is to communicate it immediately to a very delicate electrometer,

electrometer, *viz.* a very light one, that has no great extent of metallic or of other conducting ſubſtance; becauſe if the ſmall quantity of electricity that is communicated to it be expanded throughout a proportionably great ſurface, its elaſticity, and of courſe its power of ſeparating the corks of an electrometer, will be diminiſhed in the ſame proportion.

The other caſe is, when one wants to aſcertain the preſence of a conſiderable quantity of electricity, which is diſperſed or expanded into a great ſpace, and is little condenſed, like the conſtant electricity of the atmoſphere in clear weather, or like the electricity which remains in a large Leyden phial after the firſt or ſecond diſcharge.

To effect this, I uſe an apparatus, which in principle is nothing more than Mr. VOLTA's condenſer; but with certain alterations, which render it leſs efficacious than in the original plan, but at the ſame time render it much leſs ſubject to equivocal reſults. I place two of the above deſcribed tin plates upon a table, facing each other, as ſhewn in fig. 3 and about one-eighth of an inch aſunder. One of thoſe plates, for inſtance A, is connected with the floor by means of a wire, and the other plate B is made to communicate, by any convenient means, with the electricity that is required to be collected. In this diſpoſition the plate B, on account of the proximity of the other plate, will imbibe more electricity than if it ſtood far from it, the plate A in this caſe acting like the ſemi-conducting plane of M. VOLTA's condenſer, though not with quite an equal effect, becauſe the other plate B does not touch it; but yet, for the very ſame reaſon, this method is incomparably leſs ſubject to any equivocal reſult. When the plates have remained in the ſaid ſituation for the time that may be judged neceſſary, the communication between the plate B, and the

conducting ſubſtance which conveyed the electricity to it, muſt be diſcontinued by means of a glaſs ſtick, or other inſulating body; then the plate A is removed, and the plate B is preſented to an electrometer, in order to aſcertain the quality of the electricity; but if the electrometer be not affected by it, then the plate B is brought with its edge into contact with another very ſmall plate, which ſtands upon a ſemi-conducting plane, after the manner of M. VOLTA's condenſer *; which done, the ſmall plate, being held by its inſulating handle, is removed from the inferior plane, and is preſented to the electrometer: and it frequently happens, that the ſmall plate will affect the electrometer very ſenſibly, and quite ſufficient for the purpoſe; whereas the large plate itſelf ſhewed no clear ſigns of electricity.

If it be aſked, why I uſe the ſemi-conducting plane for this ſmall plate, and not for the large one? the anſwer is, firſt, becauſe the large ſemi-conducting plane is incomparably more difficult to be procured than the ſmall one; and, ſecondly, becauſe the ſmall plane may be eaſily deprived of any accidental electricity which may adhere to it; but the large one is more difficultly rendered fit for the purpoſe, eſpecially as the large plate ought in general to remain upon it a much longer time than the ſmall plate is to remain upon its ſemi-conducting plane.

The third and laſt caſe is when the electricity to be aſcertained is neither very conſiderable in quantity, nor much condenſed; ſuch is the electricity of the hair of certain animals, of the ſurface of chocolate when cooling, &c. In this caſe the beſt method is to apply a metal plate, furniſhed with an

* This ſmall plate is nearly of the ſize of a ſhilling, and the ſemi-conducting plane is of wood covered with copal varniſh.

inſulating handle, like an electrophorus plate, to the electrified body, and to touch this plate with a finger for a ſhort time whilſt ſtanding in that ſituation; which done, the plate is removed, and is brought near an electrometer; or its electricity may be communicated to the plate of a ſmall condenſer, as directed in the preceding caſe, which will render the electricity more conſpicuous. It is evident, that in this caſe the metal plate will acquire the electricity contrary to that of the ſubſtance in queſtion; but this anſwers the ſame purpoſe: for if the electricity of the plate be found to be poſitive, one muſt conclude, that the electricity of the body in queſtion is negative, and contrariwiſe. In this operation, care muſt be had not to put the metal plate too near, or in full contact with the ſubſtance to be examined, leſt the friction, likely to happen between the plate and the body, ſhould produce ſome electricity, the origin of which might be attributed to other cauſes.

Having thus far deſcribed the ſureſt methods of aſcertaining the preſence and quality of electricity, when its quantity or degree of condenſation is ſmall, I ſhall now beg leave to add ſome farther remarks on the ſubject of electricity in general, and which have been principally ſuggeſted by what has been mentioned in the preceding pages.

On the hypotheſis of a ſingle electric fluid, it is ſaid, that every ſubſtance in nature, when not electrified, contains its proper ſhare of electric fluid, which is proportionate to its bulk, or to ſome other of its properties; and it is generally believed, that this equal or proportionate diſtribution of electric fluid takes place with the greateſt part of natural bodies. However, the fact is far from being ſo; and I may venture to aſſert that, ſtrictly ſpeaking, every ſubſtance is always electrified, *viz.* that every ſubſtance, and even the various parts of the ſame

 body,

body, contain at all times more or leſs electric fluid than that quantity of it which it ought to contain, in order to be in an electrical equilibrium with the bodies that ſurround it.

At firſt ſight it may be thought quite immaterial to know, whether the electric fluid is diſperſed in the juſt proportion among the various ſubſtances which are not looked upon as electrified, or whether it deviates in a ſmall degree from that proportionate diſtribution; but it will hereafter appear, that one of thoſe aſſertions will lead us to the explanation of an intereſting phænomenon in electricity, whereas the other does not admit of it; beſides, what is called ſmall difference of the proportionate diſtribution, inſomuch as it does not affect our inſtruments, may be ſufficient for ſeveral operations of nature, which it is our intereſt to inveſtigate.

If we inquire what phænomena evince this altered diſtribution, or the actually electrified ſtate of all bodies, the preceding obſervations will furniſh ſome very unequivocal ones; eſpecially that of the doubling plates made after my plan, which ſhewed to be electrified even after having remained untouched for a whole month, during which time they had been in communication with the ground; for if each of them had contained an equal ſhare of electric fluid, the electric atmoſphere of one of them could not poſſibly occaſion a contrary electricity in the other, and conſequently no accumulation of that power could have happened.

A great number of inſtances are related in books on the ſubject of electricity, and in the Philoſophical Tranſactions, of pieces of glaſs, of ſulphur, of ſealing-wax, &c. having remained electrified ſo far as to affect an electrometer for months after they had been excited, or even touched; but the following experiment will ſhew, in a clearer manner, the great

 length

length of time that a quantity of electricity will remain upon a body.

Having constructed a gold-leaf electrometer in the nicest manner I could, and which, on account of the non-conducting nature and construction of its upper part, could remain sensibly electrified for several hours together, I communicated some electricity to it, which caused the slips of gold-leaf to diverge with a certain angle; and as the electricity was gradually dissipated, the divergency diminished in the same proportion. Now, whilst this diminution of divergency was going on, I looked through a small telescope, and by means of a micrometer measured the chords of the angles of divergency, setting down the time elapsed between each pair of contiguous observations; and as the chord of the angle of divarication is in the direct simple proportion of the density of the electric fluid *, I could by this means know how much electric fluid was lost by the electrometer in a certain time, and of course what portion of the electricity first communicated to the electrometer still remained in it. Let us make the chord of the angle of divarication on first electrifying the electrometer, or rather when first observed, equal to 16; or let us conceive that quantity of electricity to be divisible into 16 equal parts.

I observed, that when the chord of the angle became equal to eight, the time elapsed between this and the first observation was one minute; when the chord became equal to four, the time elapsed between this point and the preceeding observation was 3′ 30″; when the chord became equal to two, the time elapsed since the preceding observation was 17′; and when the chord became equal to one, the time elapsed since the

* This proposition was first ascertained by F. BECCARIA. See Philosphical Transactions, Vol. LVI.

preceding obſervation was one hour and a quarter; after which the electrometer remained ſenſibly electrified for a long time.

In repeating this experiment, the times elapſed between the correſponding obſervations did not follow ſtrictly the ſame proportion of increaſe; nor did they increaſe regularly in the ſame experiments, which may be attributed in great meaſure to the inaccuracy in obſerving, and to the fluctuating ſtate of the air; but it could be ſafely inferred from all the experiments, that the times required for the diſperſion of the electricity were at leaſt greater than the inverſe duplicate proportion of the denſities of the electricity remaining in the electrometer. And if we imagine, that they continue to diminiſh in the ſame proportion of increaſing time, which is far from being an extravagant ſuppoſition, we ſhall find, by a very eaſy calculation, that about two years after the electrometer would ſtill retain the hundredth part of the electricity communicated to it in the beginning of the experiment; and as we do not know how far a quantity of electricity is diviſible, or to what extent it may be expanded, we may conclude with ſaying that, ſtrictly ſpeaking, the electrometer would remain electrified for many years.

It may be inferred from this, as well as from many other experiments, that the air, or in general any ſubſtance, is a more or leſs perfect conductor of electricity, according as the electricity which is to paſs through it is more or leſs condenſed; ſo that if a given quantity of electric fluid be communicated to a ſmall braſs ball, one may take it away by ſimply touching the ball with a finger; but if the ſame quantity of electric fluid be communicated to a ſurface of about 100 or 1000 ſquare feet, the touching with the finger will hardly take away any part of it.

If it be aſked, what power communicates the electricity, or originally diſturbs the equilibrium of the natural quantity of electric fluid in the various bodies of the univerſe; we may anſwer, that the fluctuating electric ſtate of the air, the paſſage of electrified clouds, the evaporation and condenſation of fluids, and the friction ariſing from divers cauſes, are perpetually acting upon the electric fluid of all bodies, ſo as either to increaſe or diminiſh it, and that to a more conſiderable degree than is generally imagined.

I ſhall laſtly conclude, with briefly propoſing an explanation of the production of electricity by friction, which is dependent upon the above ſtated propoſition, *viz.* that bodies are always electrified in ſome degree; and likewiſe upon the well known principle of the capacity of bodies for holding electric fluid being increaſed by the proximity of other bodies in certain circumſtances.

It ſeems to me, that the cylinder of an electrical machine, like ABC in fig. 4. muſt always retain ſome electricity of the poſitive kind, though not equally denſe in every part of its ſurface; therefore, when the part of it A is ſet contiguous to the rubber *fg*, it muſt induce a negative electricity in the rubber. Now, when by turning the cylinder, another part of it B (which ſuppoſe to have a leſs quantity of poſitive electricity than the preceding part A) comes quickly againſt the rubber; the rubber being already negative, and not being capable of loſing that electricity very quickly, muſt induce a ſtronger poſitive electricity in the part B, which is now oppoſite to it; but this part B cannot become more poſitively electrified, unleſs it receives the electric fluid from ſome other body, and therefore ſome quantity of electric fluid paſſes from the loweſt part of the rubber to the part B of the glaſs, which additional quantity of electric fluid is retained by the part B

only

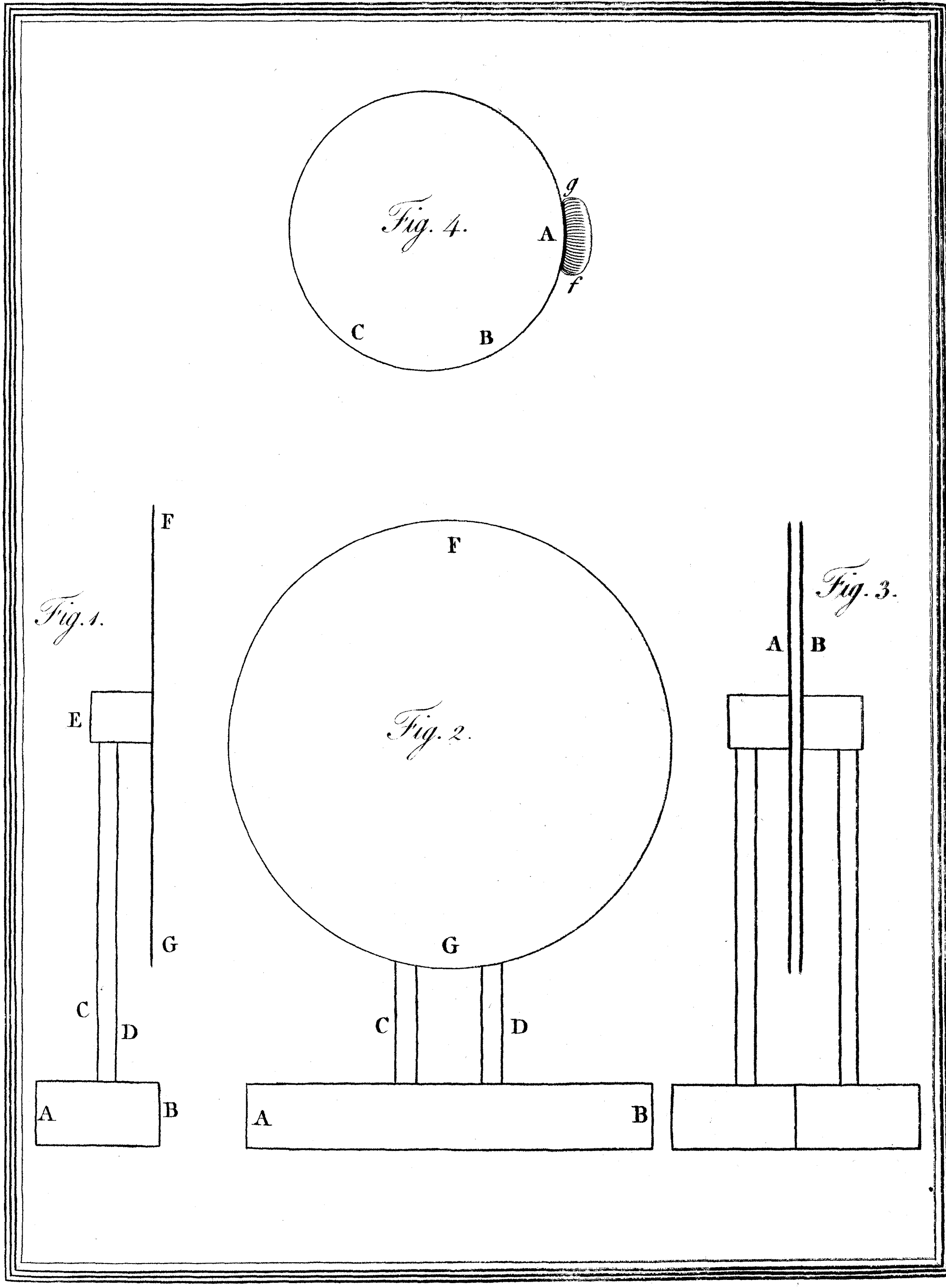
only whilſt it remains in contact with the rubber; for after that, its capacity being diminiſhed, the electric fluid endeavours to eſcape from it. Thus we may conceive how every other part of the glaſs acquires the electric fluid, &c. and what is ſaid of the cylinder of an electrical machine may, with proper changes, be applied to any other electric and its rubber.

It appears, therefore, that according to this theory, a part of the rubber, *viz.* that which the ſurface of the glaſs cylinder enters in turning round, muſt ſerve to furniſh the electric fluid to the glaſs, and the upper part muſt be poſſeſſed of a negative electricity capable of inducing a poſitive electricity in the glaſs contiguous to it. In fact, this ſeems to be confirmed by the general practice and experience; for that rubber anſwers beſt for an uſual electrical machine, which can eaſily conduct the electric fluid with its under part, and the upper part of which is more ready to acquire, and to retain, the negative electricity; hence the rubbers are generally furniſhed with amalgam below, and with a piece of ſilk above; hence alſo, if the cylinder of the machine be turned the contrary way, it will produce little or no electricity.

It often happens, that the part which conducts the fluid, and that which acquires the electricity contrary to that of the electric, are not ſo diſpoſed in a rubber as the one above deſcribed; but it remains always true, that the rubber muſt be poſſeſſed of thoſe two properties, *viz.* to conduct the electric fluid very readily in one or more parts, and to acquire, as well as retain, on other parts, an electricity contrary to that acquired by the electric that is to be rubbed with it.

Little St. Martin's-Lane,
Nov. 7, 1787.

T. CAVALLO.



T. Cavallo del.